

THE PRATT-WOODRUFF EXPERIMENT: REPLY TO DR. PRATT'S COMMENTS

By CHRISTOPHER SCOTT

1. After completing the first draft of our paper, Medhurst and I added a note saying that "the hypothesis of card misplacement by the experimenter does not necessarily imply conscious deception."

Pratt has taken this to imply that we were supporting the hypothesis of unconscious manipulation, but our purpose was merely to point out, in fairness to Woodruff, that this hypothesis was not necessarily excluded. Pratt has now produced some evidence against this hypothesis and I do not intend to dispute his conclusion. This seems to leave us with a straight choice between deliberate misplacement and ESP. It is perhaps fortunate that this position should have been clarified by Pratt himself, who cannot be suspected of siding with Hansel.

2. Pratt suggests a number of tests, some involving new analyses, others implicit in results already published, based on the idea that the hypothesis of unconscious card misplacement would lead one to expect a rise in the scoring level as the experimenter learned to recognize the cards from their backs. This would not apply to conscious misplacement, where the cards are supposedly identified simply by turning them over and looking at them. Thus these tests do not test whether manipulation occurred but only whether *unconscious* manipulation occurred. I make no apology for our not having carried out such tests: in my view the question whether any manipulation that may have occurred was exercised consciously or unconsciously is of no scientific significance and does not justify research effort.

3. The only other test Pratt suggests is a run-by-run correlation between the percentage of hits found in the E- and M-piles. On his own admission, however, this does not in itself give the required discrimination between ESP and manipulation since both hypotheses would lead to a positive correlation. Following his suggestion I have carried out this test on the P.M. data and obtained a correlation of 0.2277 (154 pairs), which is significant at the level $P < .01$. It is not clear how to proceed from here. Pratt suggests that a computer simulation might be used to test whether the observed correlation

could arise on the misplacement hypothesis. Unfortunately there seem to be too many unknown parameters to allow any firm conclusion. P.M. was scoring at an average deviation of 1 per run. This might have been achieved in theory by making n misplacements every n th run, for any value of n . The higher the n , the higher the correlation to be expected; but we do not know n and I see no hope of estimating it to a useful degree of precision. Further, the misplacement could have been in favor of E-hits only, or against E-misses in addition; we do not know which, but the choice of model affects the correlation to be expected. After working on this problem for many hours, I am personally fairly certain that no approach along these lines can be made to yield a firm conclusion; there are simply too many unknowns. But I could be wrong, and perhaps some other statistician reading this note may prove more resourceful. I would be happy to pass on the data to anyone willing to make the attempt, or I would carry out myself any tests which anyone could suggest—provided only that the suggestion were to be accompanied by a reasoned statement of the way in which significant inferences might be drawn from the results of the test.

4. This exhausts the tests suggested by Pratt. He complains that we failed to carry out any new tests. In fact we carried out two (plus a third abortive one described in an earlier draft of the paper which he saw, which I later excised under editorial incitement to brevity). I assure him that our failure to perform any other test was not due, as he supposes, to lack of concern but simply to our inability to imagine any further fruitful tests. In this respect I cannot see that he has improved on our performance. As I have shown, none of his proposed tests throws any useful light on the essential issue, which is simply ESP versus manipulation.

5. Pratt surprisingly suggests possible bias in our decision to test *all* the data from the successful subjects. Paradoxically he feels that we would have been more impartial if we had limited our analysis to runs scoring 6 and over. But why 6? We could have chosen 5, or 7. The choice would have left the door open to a charge of bias. The best defense, we felt, was to choose all. We thought we had explained this clearly enough. However, to set his doubts at rest I have now repeated our analysis for the 6-and-over runs. Results are as follows.

Subject	Ratio Mean Score on E-Piles to Mean Score on M-Piles		χ^2 (1 df)		<i>P</i> (One-tailed)	
	Runs Scoring 6+	All Runs	Runs Scoring 6+	All Runs	Runs Scoring 6+	All Runs
P.M.	1.47	1.40	18.94	21.36	10^{-5}	2×10^{-6}
D.A.	1.22	1.19	5.23	5.12	.011	.012
H.G.	1.06	1.06				
C.C.	1.09	1.03				
D.L.	1.25	1.19				

The focus of our analysis was, of course, on the four subjects D.A., H.G., C.C., and D.L. The above figures show that "our" case would have been slightly strengthened if we had selected the data in the way Pratt would have preferred. This seems to be a complete answer to the charge of selection bias.

6. Pratt writes: "Since Medhurst and Scott say that Hansel was wrong in claiming significance . . . in his book, it appears that they must have made a separate study of the runs with scores of 6 and above. Why did they not report these results? [p. 190]." Medhurst did indeed make such a study (long after we had obtained the results reported in our paper) in an attempt to duplicate Hansel's findings. The results were confused by the uncertainty as to the exact identity of the BSTM runs. However, it seemed clear that there were errors in Hansel's data, and Medhurst was unable to confirm the significance level reported by Hansel. (Pratt is mistaken in claiming that we say in the paper that "Hansel was wrong in claiming significance." We do not say this. Hansel claimed a *P* of less than .01, while Medhurst found $P = .02$.) We did not report the findings of this analysis for the simple reason that the method used (Hansel's) was crude. Our own analysis was more sensitive and made Hansel's superfluous. The answer to Pratt's question beginning "Were their findings . . . [p. 190]?" is a simple "No." I may add that Medhurst's analysis of the 6-and-over runs did not give us the results quoted earlier in the present note or anything comparable, since the method of analysis is quite different. My first knowledge of the latter results was when I computed them for the purpose of the present note.

7. Pratt says that our hypothesis has not been established. Whatever hypothesis he thinks of as "ours," this is unquestionably true and I am glad of this opportunity to stress the fact. No hypothesis about the Pratt-Woodruff experiment has been established.

8. Pratt's suggestion that the authors "were sympathetic with Hansel's general point of view" [p. 188] is pure fantasy in the case of Medhurst, the senior and more active author. The late Dr. Medhurst's review of Hansel's book (Medhurst, 1968) is surely sufficient evidence.

9. Pratt's suggestion that variations in scoring success among the subjects are not to be expected on the card-misplacement hypothesis is surprising. The misplacement method depends on the subject giving visual and aural cues to the experimenter regarding placement of the cards on the pegs. Clearly, variations in this respect would be expected from one subject to another.

10. The suggestion that Hansel reached his conclusion by a "groping" approach—noticing first the E-pile effect in the data and then inventing a hypothesis to fit it—makes no sense unless there were available to Hansel a fair number of explanatory hypotheses to choose between, each consistent with the experimental set-up. If there were, then obviously the experiment is no good as evidence of ESP. The striking thing about Hansel's finding is that his hypothesis is the only one yet published (and, so far as we know at this stage, the only plausible one that can be thought of) which *could* explain the successful scores without ESP, and that this *apparently unique* hypothesis also fits the E-pile finding.

11. In their reply to Hansel's paper Pratt and Woodruff (1961) repeatedly stressed the failure of Hansel's attempt to *confirm* the P.M. findings in the data of the other successful subjects. ("Confirmation of the effect in the data of other high-scoring subjects was therefore of paramount importance [p. 124].") Yet, now that we have shown that (despite errors in Hansel's work) the confirmation does exist, Pratt turns around and says that it makes no difference. We are not saying that our evidence proves Hansel's hypothesis; we are saying that it moves the balance at least some distance further towards Hansel's hypothesis. I do not see how this can be rationally denied.

12. In reference to Pratt's postscript I would like only to say that I fully agree that we, as Dr. Woodruff's critics, owed him the same courtesy as he extended to us. I believe we accorded him this, and I am not aware of having withheld any information from Woodruff which was available to me and relevant to our article. I agree with Pratt that Woodruff's failure to reply to our paper should not weigh at all in evaluating the balance of probabilities as regards the interpretation of this classical experiment.

REFERENCES

- MEDHURST, R. G. The fraudulent experimenter: Professor Hansel's case against psychical research. *Journal of the Society for Psychical Research*, 1968, **44**, 217-32.
- PRATT, J. G., & WOODRUFF, J. L. Refutation of Hansel's allegation concerning the Pratt-Woodruff series. *Journal of Parapsychology*, 1961, **25**, 114-29.

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